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Current issues and new directions in *Psychology and Health*. The role of the critic in health psychology: The Healthy Scepticism Project

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EDITORIAL
Current issues and new directions in *Psychology and Health*. The role of the critic in health psychology: The Healthy Scepticism Project

My colleagues and I have been involved in a Healthy Scepticism Project in which we model for anyone interested how to assume the role of the critic in scrutinising claims occurring in the health psychology literature, claims that often get further hyped in the popular press. Health psychology, in sharp contrast to biomedicine, is markedly lacking in published post-publication criticism of what appears in its journals, although prominent medical editors believe that such criticism is vital to the integrity and credibility of their field (Horton, 2002). Compared to their frequent appearance in the medical journals, letters to the editor criticising published studies are much more uncommon in health psychology journals. Could it be that peer review is that much better developed in health psychology, so that pre-publication peer review eliminates the need for post-publication criticism? We doubt that is the case and document this with troubling examples from the health psychology literature.

We do not assume that the authors we criticise are incompetent or deliberately misleading the field. Rather, we assume that some well-entrenched myths about the powers of mind over body are self-perpetuating. These myths are intimidating and compel agreement, particularly when there is the precedent of their having been restated repetitively, especially in the discussion sections of articles in which the data that are reported contradict the myths. These myths often have strong support from the larger culture, so that they walk away unscathed from any High Noon showdown with disconfirming data. As my Italian grandmother frequently said: '*Se non è vero, è bene trovat*', which she loosely translated as 'If it is not true, it ought to be'. But we do not think the field benefits from perpetuating noble lies and fairy tales about mind–body relations or the power of psychological interventions. Indeed, the field loses even more credibility when errors inevitably get found out. Recall that it was the excess of proponents of psychological factors in medicine, not the activities of critics from within the field that drove the psychosomatic medicine of the 1940s and 1950s into discredit and near oblivion (Holroyd & Coyne, 1987). My colleagues and I believe we are doing the field a service in breaking the spell of these myths, even if we cannot consistently expect expressions of gratitude.

In the Society of Behavioral Medicine-sponsored Great Debate on the efficacy of psychosocial interventions for cancer patients (Coyne, Lepore, & Palmer, 2006), we showed that there was a consistent best-foot-forward, confirmatory bias in reporting of the results of clinical trials. Benefits of interventions were exaggerated, and the myth was being perpetuated that cancer patients who are not distressed could nonetheless obtain a reduction in distress as a result of an intervention.

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Worse, when investigators did not engage in a cover up, but honestly and transparently reported null findings, they risked their readership dismissing the efficacy of their interventions. Health psychology has bought into the myth that interventions could be fairly tested with patients who actually were not distressed enough to register an effect. Yet, if inclusion criteria involved patients having elevated distress, positive results might be obtained.

Next, we took on the popular myth that psychotherapy promotes the survival of cancer patients, a myth perpetuated by misinterpretation of a classic study by David Spiegel and colleagues (Spiegel, Bloom, Kramer, & Gottheil, 1989). We showed that there was nothing extraordinary about survival in the intervention group in that trial: Most subsequent studies approximated its survival curve in *both* their intervention and control arms (Coyne, Stefanek, & Palmer, 2007). What was fascinating, but generally overlooked, was that no one had ever been able to replicate the survival curve for the small ($n = 36$) control group from the original study, with its odd sudden accumulation of deaths 2 years after the start of the study. Interpretation of a randomised controlled trial requires the assumption that patients in the intervention group would have obtained the same outcomes as patients in the control group, if they had not received the intervention. This assumption did not hold for the Spiegel et al. trial. We cannot determine why not, except that such flukes are more common when large effects are claimed for small, grossly underpowered trials, and especially when the focus is on an outcome that was not targeted in the original design of the trial. In our systematic review, we found that no trial, including Spiegel's, had ever found that psychotherapy improved the median survival time of women with metastatic breast cancer. Furthermore, no trial in which survival was chosen as the primary outcome ahead of time ever demonstrated a survival effect for patients with any type of cancer, when psychotherapy was not confounded with better medical surveillance or treatment.

Not long afterwards, press releases proclaimed that psychosocial intervention resulted in breast cancer patients with local progression having fewer recurrences and deaths and longer recurrence-free and survival intervals. Had we been premature in our dismissal of effects of psychotherapy on biomedical outcomes? We critically examined the trial on which these claims were based (Andersen, Yang, & Farrar, 2008) and found that multivariate statistical analyses involved inappropriate overfitting of regression equations, with too many covariates given the sample size, increased the likelihood of spurious positive results (Stefanek, Palmer, Thombs, & Coyne, 2009). Although the trial was not reported in a way that readily allowed examination of basic statistics, it appeared that no effect was obtained for recurrence or survival. If one controlled for multiple comparisons, there was no effect of the intervention on at least eight measures of mood, 15 measures of immune function, four measures of social functioning and four measures of adherence to chemotherapy that were examined. Our re-assessment of the trial contrasted sharply with the press releases and original reports of the trial, but so far, no rebuttal has been forthcoming.

Such debunking of classic and contemporary claims in the health psychology literature emboldened us, and we kept looking. What we uncovered was shocking, even given our high level of scepticism. We examined four meta-analyses of psychosocial interventions, each commissioned by the Society of Behavioral Medicine's Evidence Based Behavioral Medicine Committee and having a senior member of the field among its authors (Coyne, Thombs, & Hagedoorn, 2009). We found numerous lapses in transparency and notable inconsistencies between

what was reported in the original studies and the data entered into the meta-analyses, misclassification of studies, miscalculation of effect sizes and outright contradiction of the authors' interpretations of the meta-analyses by the results they had obtained. We concluded that none of the four meta-analyses provided a suitable basis for clinical and policy decisions. The authors we criticized were offered an opportunity to provide replies to accompany our article, but as yet, none has been forthcoming.

We next requested supplementary tables for a comprehensive meta-analysis of stress-related factors in cancer incidence, survival time and mortality (Chida, Hamer, Wardle, & Steptoe, 2008). Most of the studies reported null effects, and as a group, they were generally of poor quality, with basic analyses lacking appropriate statistical controls. Yet the effect size for the meta-analysis for cancer mortality stood out, with a hazards ratio of 1.29 (95% CI 1.16–1.44). This was still not of the magnitude generally assumed by epidemiologists to be of public health or clinical importance, but we were suspicious (Coyne, Ranchor, & Palmer, 2010). Examining the supplementary tables kindly provided by the authors, we found most studies were null when they included appropriate statistical controls, with HRs clustering around 1.0, with the striking exception of HRs ranging from 23.8 to 74.2, all from the work of Grossarth-Maticek and colleagues examining the causation of cancer by personality (Grossarth-Maticek, Bastiaans, & Kanazir, 1985; Grossarth-Maticek, Eysenck, & Vetter, 1988). These are extraordinarily strong associations. Unfortunately, this work was subject to exhaustive critical scrutiny 20 years ago and now generally is seen as fraudulent (Van der Ploeg, 1991). Worse, Hans Eysenck, one of the co-authors of some of these studies and editor of the journal where some were published, was revealed to have received an undeclared 800,000 British pounds from American tobacco companies (Pringle, 1996) to demonstrate that the link between smoking and lung cancer was spurious: personality was related to smoking and personality was related to lung cancer, but any apparent strong direct relation of smoking to lung cancer was illusory. We are now working to document the persistent influence of these discredited data on discussions of a stress-cancer link.

As might be expected, not everyone is enthusiastic about our critical re-analyses and outright muckraking. The president of my university received two letters complaining about my work, with one requesting my suspension. Twice I have had the uncomfortable experience of being debated in absentia in a key note address at Society of Behavioral Medicine conferences. A reviewer of our review of the four meta-analyses of health psychology interventions demanded that our manuscript be rejected, strongly objecting to its tone and bitterly complaining that we had assumed the role of 'Supreme Judges of the work of others – however flawed this work might be'. As often occurs with our work, the editor had to intervene to protect us from upset reviewers. Although there have been notable exceptions, with some coaxing, most editors can be convinced that we are providing a service to the field and that they should protect us from vengeful reviewers.

The most principled objection to our line of work is that it is embarrassing and detrimental to the credibility of a field already not being taken seriously by our colleagues in the hard sciences and biomedicine. Yet, we do not believe that greater credibility is to be obtained by stifling criticism, but by visibly enforcing high standards. My colleagues and I have taken some heat, but we hope that we have encouraged others to take up the challenge of maintaining a healthy scepticism and pointing out lapses in the quality of work. We owe this to ourselves as a field and to

the policy makers and our patients who look to the health psychology literature for guidance in their decision making.

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